1979–

THE KERR YEARS

Challenges and Prospects

by Donald M. Kerr

On this occasion of the 40th anniversary of the founding of the Los Alamos Laboratory, I would like to shape in broad outline my hopes for the Laboratory in the next decade. Though some of what I will say may go beyond what might be labeled as realistic, we must have such high hopes, for they help us stretch our capabilities. I will also address some substantial obstacles that could, if not countered, negate our best attempts to help the nation solve some of its pressing problems.

My first hope is that Los Alamos scientists will play a prominent role in reshaping the defense posture of America through efforts along three lines—arms control, nuclear weapons, and advanced weapons concepts.

The people of this planet have no more important task than to subdue the spiraling arms race and to eliminate the fear that, by accident or by design, nations might eliminate large portions of life on this earth by engaging in a massive nuclear exchange. While science cannot solve the political problems that snarl arms control talks, improved technology in satellite surveillance, seismic detection, and information analysis can help decrease the possibility of agreement violations through surprise actions, clandestine activities, or new developments. Such technological assistance is not likely to be the key element in advancing attempts to curb the arms race but may be useful if political developments become favorable.

Our nation's efforts toward arms control must be made from a position of strength. And that strength depends on being at the forefront of all scientific areas likely to yield new military applications. In the area of nuclear weapons, Los Alamos can make the following specific contributions.

- Encourage the modernization, where appropriate, of nuclear warheads to provide the best safety and security features technology can offer.
- Assure the effectiveness of nuclear weapons over a wider range of operating conditions.
- Improve the protection of warheads against newly developed electronic countermeasures designed to defeat our weapons.
- Develop new means of making our weapons more effective

against hardened targets in the Soviet Union.

- Improve the techniques for defending our own strategic forces from a first strike.
- Determine the feasibility of newer weapons, including those involving particle beams and lasers.

Finally, Los Alamos can contribute to the nation's defense through efforts in what we call advanced weapons concepts. This Laboratory was created to meet what was viewed as the most critical defense issue facing the country in World War II—the possibility that our enemies were developing a weapon based on new science and new technology. It is vital that the critical military needs currently facing the nation be met in a similar fashion today.

One advanced weapons development would be the introduction of truly intelligent weapon systems to the battlefield. Such systems have been discussed and popularized, but the immensely difficult task of developing them, although possible, remains to be done. I have in mind a weapon system including multiple sensing techniques coordinated by sophisticated electronics and computing capabilities. The intelligent weapon system would be integrated into an overall battlefield posture involving land, sea, and air forces.

Ten years or so ago the prospects for artificial intelligence were oversold, and work in that area received a bad name. But significant developments over the past decade suggest that now is the time to initiate its application. Already a number of techniques for using computers as expert systems are in the early stages of application. For example, one computer manufacturing company is using a modest form of artificial intelligence to establish the appropriate configurations of computer systems for purchasers. A computer programmed with more than two thousand rules and fed the requirements of the purchaser determines the configuration of equipment that best meets those requirements. Another and perhaps the most widely noted example is the use of computers in medical diagnosis to help physicians make the complex judgments required of them when faced with multiple symptoms and test results. In over 95 per cent of the tests thus far, diagnoses made by the computer agree with those of expert physicians.

The eventual goal in a military context is a weapon system that can be sent into a battle situation to sense and analyze many complex, perhaps rapidly varying factors, such as terrain, environmental conditions, and the nature and movement of enemy forces and weapons. The system, controlled by artificial intelligence, would make the decision as to which of its weapons to deploy and in what manner they would best be utilized. Such a system may sound far-fetched to some, but the technology required has progressed to the point that it should be vigorously pursued.

A nation possessing an intelligent weapon system would have a great tactical and psychological advantage over its enemy. Furthermore, smart weapon systems equipped with today's advanced nonnuclear warheads could displace low-yield, short-range nuclear weapons and thus considerably reduce the tension associated with the posting of nuclear weapons close to an enemy's borders.

Research along these lines should be pursued, and Los Alamos, together with Livermore and Sandia, can make important contributions in the next ten years, if properly supported and freed of extensive program strings, milestones, and reporting requirements. Modest funding of a few million dollars per year to each of the weapon-related national laboratories would be a sufficient beginning.

There are many other exciting advanced weapons concepts; I will mention only a few. We have ideas for antiterrorist technology that could reduce the impact of threats in many areas. We see means for detecting and protecting against chemical and biological threats. And we see a possibility of developing microwave weapons, which could become very important as electronics becomes more and more integrated into the battlefield.

My second hope is that the Laboratory will make major contributions to solving a problem that has commanded great public attention—the problem of supplying the energy needs of the nation and the world. The Laboratory has devoted a substantial effort to energy programs during the past decade, and it is my hope that as these efforts reach maturity in the coming decade, they will bear technological fruit in the following forms.

- Safety and engineering advances that will make nuclear power a more acceptable approach when the world turns again to this energy source, as I believe it eventually will.
- Nuclear waste disposal techniques that will satisfy public concerns.
- Techniques for extracting fossil fuels from the earth that will provide greater efficiency and worker safety and cause less pollution and environmental damage.
- Practical fuel cells that will power many diverse activities, from transportation to materials production.
- Geothermal projects that will tap the heat of the earth's mantle to provide a clean and safe supply of heat and electricity.
- Advances in renewable energy technologies that will allow for decentralized energy supplies so necessary in rural America and in many developing nations.

Controlled fusion is a major area in which we have made and continue to make important contributions to the development of a new energy source for future use. Since the early 1950s Los Alamos has played a major role in the international development of magnetic confinement science and technology. This cooperative effort has led to such a high level of sophistication that demonstration of energy break-even, using the mainline tokamak approach, seems assured during this decade. The ability to confine reactor-grade plasmas for times close to those required for thermonuclear ignition is an enormous scientific accomplishment that could not have been achieved without the resources that national laboratories, universities, and industry brought to bear on this problem.

At the same time it is clear to me that the demonstration of scientific feasibility on the tokamak will not automatically assure its economic feasibility as a power-producing system. It is likely that proof of commercial feasibility will fall to a different fusion concept whose inherent confinement requirements reduce engineering complexity and therefore cost to the point where it can become a practical system for the nation to adopt, or perhaps commercial feasibility will fall to much more advanced tokamak systems yet to be developed.

I believe the work going on at Los Alamos will play a significant role in developing a power-producing fusion reactor. I am encouraged in this respect by recent successful developments in our Reversed-Field Pinch and Compact Toroid programs because the efficient confinement properties of these schemes provide the magnetic fusion program with a new possible end-product: the compact, high-power-density reactor. This new approach efficiently utilizes resistive copper magnets and therefore differs qualitatively from the conventional reactor models, based on superconducting magnets, in greatly reducing the size, mass, complexity, and cost of a reactor and the time required for reactor development. These alternative fusion concepts are at an earlier stage of scientific development than the tokamak. Their potential for resulting in a significantly better commercial product provides the rationale for support in a well-balanced and prudent national program. Ideally, in such a program the allocation of resources will permit the full potential of these alternative concepts to be realized so that their best reactor attributes can merge with the more mature development base for the mainline approach to produce an optimized fusion system.

Diverse funding of numerous approaches is the best means for overcoming the great technical challenges posed by controlled fusion. If such funding occurs, I believe that Los Alamos can develop fusion power systems that are smaller, cheaper, and more easily maintained. Such developments may enhance the willingness of society to adopt this form of technology.

My third hope concerns the application of the Laboratory's expertise in physics, chemistry, and engineering to the new challenges in the fields of biology and medicine. Two instruments of fundamental importance to biomedical research have been developed at Los Alamos. These are the liquid scintillation spectrometer, which makes possible simultaneous counting of different radioisotopes, and the flow cytophotometer, which allows rapid analysis and isolation of individual cells. The latter development resulted in the establishment at Los Alamos of the National Flow Cytometry Resource. Current activities give me confidence that the next decades will see developments of similar importance to biology and medicine.

For example, improvements in flow cytometry now allow rapid identification and separation of chromosomes. This capability, coupled with powerful recombinant DNA techniques, opens new approaches in cell biology and genetics. The chromosome rearrangements characteristic of tumor cells can now be closely scrutinized, and this information may provide insight into the origins and abnormal behavior of cancer cells. With similar techniques cultured plant cells may be manipulated to produce new crop varieties with desired genetic characteristics, such as disease resistance and environmental tolerance.

Another example is the development of noninvasive techniques for analyzing human functions with minimal discomfort to the patient. In one such technique a nuclear magnetic resonance coil is used to follow the course of metabolic processes from outside a patient's body. The coil detects important intermediate products of metabolism that have been labeled with a suitable magnetic isotope, such as carbon-13. The labeled materials are available from the Laboratory's Stable Isotope Production Facility, which pioneered in the field of stable isotopes for biomedical research.

The Laboratory is also developing advanced physical techniques for biological and medical applications. Examples include rapid, precise identification of microorganisms based on their scattering of circularly polarized light and detailed structural analysis of biological macromolecules based on neutron and x-ray spectroscopy.

Another venture into the realm of biology exploits our computing capability—the largest in the world—to compile and make available to the scientific community a library of genetic sequences. Los Alamos has recently been designated as the site of the national DNA sequence data bank. This data bank will contribute significantly to unraveling the mysteries of DNA.

The Laboratory has a major responsibility in developing secure alternative energy sources such as shale oil. Experimental shale retorts and advanced capabilities in cellular and genetic toxicology provide the opportunity to choose extraction and processing methods that produce the least harmful pollutants. This will involve using the advanced techniques described above to study the effect of pollutants on cells.

It is my hope that, with strong inputs from academia and industry, the advanced physical, theoretical, and computational capabilities of Los Alamos will contribute to a decade of imaginative and striking benefits in the areas of biomedical research, energy development, and environmental science.

My fourth hope is that the Laboratory will continue to involve an increasing number of scientists from universities and industry in its activities. We have already made great progress in this area by establishing three centers designed to reach aggressively beyond our borders: a branch of the Institute of Geophysics and Planetary Physics, the Center for Nonlinear Studies, and the Center for Materials Sciences.

In terms of new efforts, I see the following possibilities.

- That not one but two or three of the world's most powerful computers will be available beyond the bounds of our security fences for use by collaborating scientists from other institutions.
- That more and more students and faculty will become familiar with our activities and facilities by choosing to pursue research at Los Alamos.
- That our staff will increasingly aid in the transfer of technical information to industry and to universities by sharing in joint exchange appointments.

It is, of course, impossible to mention all significant advances expected in a laboratory as diverse as Los Alamos. But one final hope is that we will be surprised by some unexpected development or discovery that derives from the exploration of new questions and new possibilities. The very nature of scientific research makes such surprises possible, and for this reason basic research is a fundamental element in our plans.

To realize the hopes that I have outlined, difficult scientific problems will have to be confronted, pursued, and conquered. But those efforts now face challenges beyond the inherent scientific difficulty.

A changed political and social climate challenges these hopes. Some voices now question the major mission of the Laboratory. They ask, "Why is the Laboratory still engaged in weapons work?" That question often comes from those who believe that the thousands of nuclear warheads now in our arsenal are more than adequate and that no more effort in this scientific area is needed. These people deserve a reply.

Three chief factors drive our continued efforts in weapons. I touched on two of these above but their importance leads me to reiterate. The first is the extent to which potential enemies of the United States are making technological advances that could jeopardize the defense posture of the United States. This issue led to the creation of the Manhattan Project during World War II, and it is still a valid concern in the present political climate. Our political leaders generally feel that their ability to influence world affairs is affected by the extent to which the United States maintains technological supremacy in the defense area.

The second factor is the need for solutions to technical problems that may inhibit accords on arms control. Any agreement on this subject rests heavily on the ability to determine that its provisions will be followed by each signatory. The inability to verify compliance has created stumbling blocks in past negotiations. The Laboratory must assist in developing new verification techniques, for they may be a critical link in reaching the goal of arms control. The Laboratory will also be called upon to help policymakers understand the capabilities and limitations of current approaches to verification.

The third factor is the certain knowledge that the pursuit of science inevitably yields ideas for new technologies that have a wide variety of applications, including military ones. The choice to develop the new military applications is the nation's. But the nation cannot choose to stop the scientific effort that creates those applications without also stifling development in other human endeavors. Science is neither compartmentalized within itself nor isolated from its surroundings. New scientific ideas have a way of leaping traditional boundaries among fields of science and of creating vast and unforeseen changes in the economic and political fabric of society.

Another challenge facing the Laboratory is the idea of some that our research activities be transferred to academia and industry. You might ask, "What is the place of Los Alamos in the midst of the country's large and sprawling research community?" After all, research efforts at universities have grown substantially since World War II, and industry has also seen reason to invest in research and development.

I believe there is a clear place for Los Alamos and other national laboratories. That place goes beyond weapons work, which the government obviously must control directly, to other areas of research in which a strong national interest justifies the presence of a federally supported laboratory.

For example, many areas of research—a notable example being nuclear fusion—face such inherent difficulties that they will yield results only over a very long term. Industry will not be inclined nor financially able to enter such areas. Another example is the area of research on the protection of workers, the public, and the environment from technologies new or old. Here the profit motive of industry may bring into question their objective assessment.

National laboratories such as Los Alamos can address these issues, and, in fact, Los Alamos is extraordinarily well equipped to do so. Our scientific computing capabilities are unsurpassed. We have the experience of dealing with military agencies and understand their needs and procedures. We can work in a way sometimes referred to as vertical integration: that is, we can develop an idea for, say, an instrument all the way from conception to production engineering. Our activities range from undirected basic research to production engineering of devices that weigh tons. We can transform ideas or bits of Nature's secrets into products useful to mankind. Of the thousands of laboratories in the nation only a small handful match this Laboratory's capabilities.

The world is increasingly specialized, compartmentalized, separated into isolated parts. The concept of integrated teamwork bringing mathematicians, physicists, chemists, biologists, engineers, and economists together for a sustained effort is not a tradition at very many institutions. In fact, it seldom happens. It is difficult to bring about. In many places it is impossible. At Los Alamos it is the usual practice. It is the way we have conducted business from the beginning.

The third challenge facing the Laboratory in the next decade concerns the level of financial support for its activities, particularly for basic research. Funding reductions can harm our work in important ways, and basic research often suffers more harm than other areas because sponsors are inclined to view it as less important than work closely coupled with an approaching milestone.

In the mid 1970s Congress established a new budget process in recognition of the need to review federal economic policy and to reduce the federal deficit. The resulting tighter budgets and economic policies have affected virtually all the Laboratory's activities and present a most serious challenge. My hopes for Los Alamos cannot be realized unless increased funding is available. The requested increases are modest but essential and represent a valuable investment for the nation.

The Laboratory is being asked to make sure that its work in major programs connects directly to program objectives that will yield usable technological applications. This emphasis must not be overdone, and in some cases that line has already been passed. When investigations have reached the stage at which such requests are appropriate, the emphasis may help us do what we want to do—to show that our work can solve national problems and lead to benefits for the nation and the world.

But we must constantly guard against demands for immediate, practical benefits from science. When basic questions are still being explored, when answers are only beginning to appear, and when technological applications are only dimly perceived, then questions of practical benefit must be deferred. If we at the Laboratory do our job well, we will open new areas of science that eventually will yield benefits. The nation must allow competent scientists to explore those areas and to confront the difficulties that may take years to overcome, satisfied that this investment is worthwhile. Budgetary restraints must not be allowed to force out all but research that is immediately applicable, for that course would amount to eating the seed corn of future harvests.

Let me conclude with a final challenge—the desire of some that science should overcome the tangled web of politics and assure that all its results are used only in positive ways. Such a desire is natural, but it is too much to expect of any single sector of society.

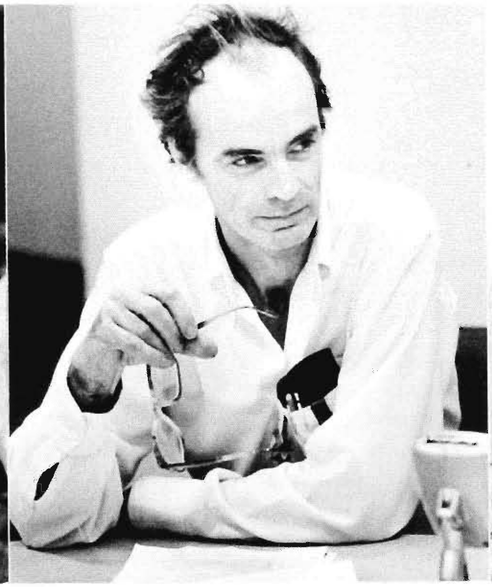
At the end of World War II, those at Los Alamos learned with the rest of the world that technical developments were beyond the control of the small group of scientists who pleaded that the results of their work be used solely for peaceful purposes. That control rests with the broader institutions of society. Today we continue to pursue the unanswered questions of science in the belief that our efforts will enhance the peace and prosperity of the world. The ultimate hope of those of us at Los Alamos is that the voices for peace will prevail in all decisions that affect the use of our endeavors. ■

What's Happening Now...

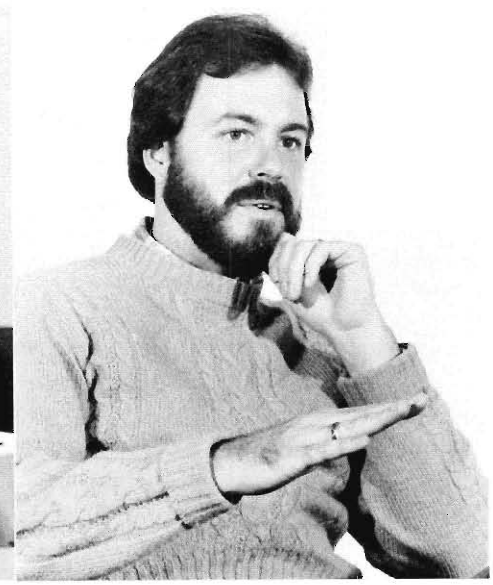
What better way to learn about the state of the Laboratory—its present excitement and its future possibilities—than to talk with some of the outstanding scientists at Los Alamos. We chose ten who represent a wide spectrum of fields and asked them to share their personal views on the mission of the Laboratory, the current work, the management of research, and some pragmatic directions for the future.

SCIENCE: *I know that many of you chose to come to Los Alamos for personal reasons and are enthusiastic about its setting, its people, and your own work here. But Los Alamos has always been a*

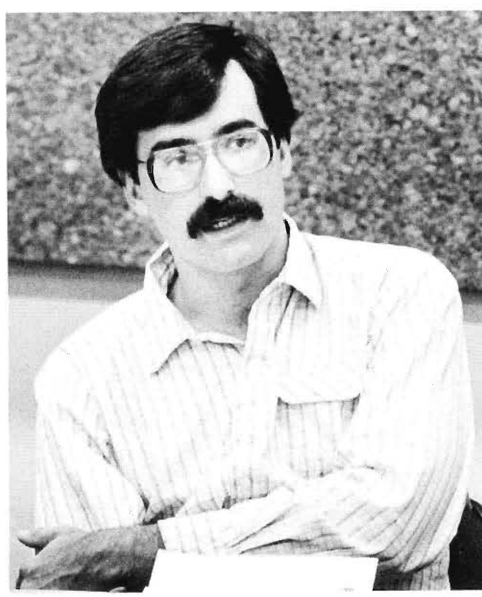
Dan Baker



Stirling Colgate



Brian Crawford



Rocky Kolb

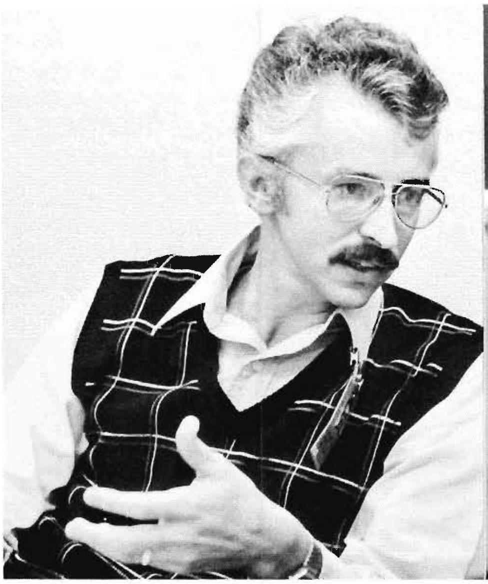
Jeremy Landt

mission-oriented Laboratory, and I wonder how you view that mission and your role in it?

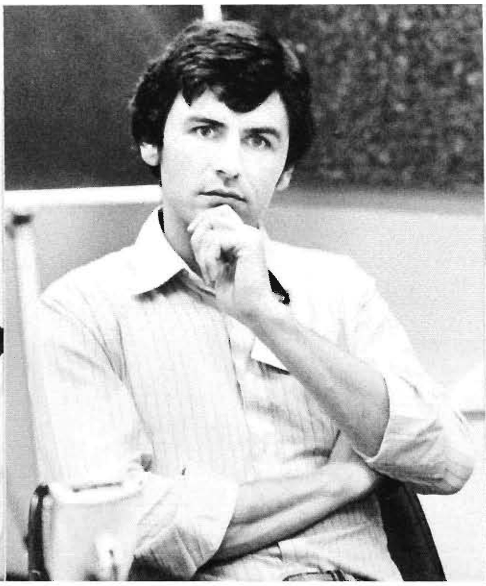
BAKER: Let me suggest a definition of the main mission of the Laboratory. Our mission is to provide input on all energy and national security issues that have a scientific or technological component. Is that general enough?

WHEATLEY: Yes, but I wonder whether the Laboratory's management has firmly in mind what technologies and ultimate applications we should be seeking.

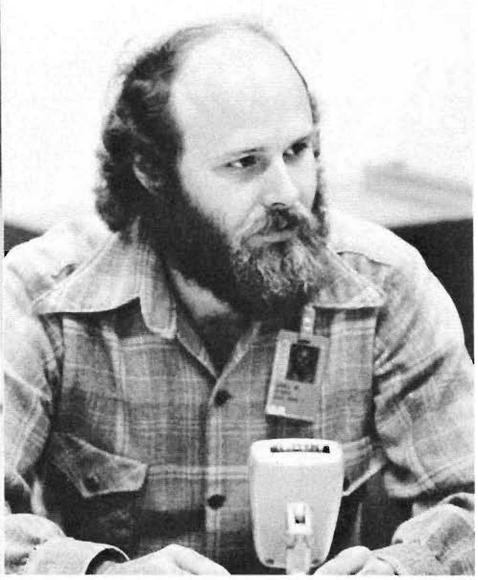
HECKER: I personally feel that national security is our most important mission. Essentially, the country has entrusted to us and to Livermore their nuclear defense.



Sig Hecker

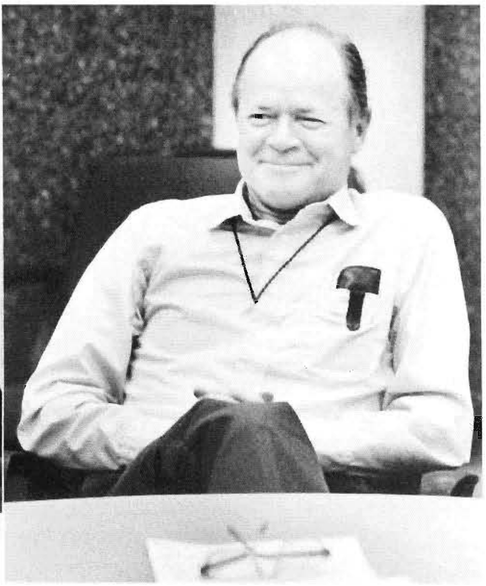


Steven Howe



Mac Hyman

Steve Rockwood



John Wheatley

Dan Baker on Space Science

The Vela satellite program to detect nuclear explosions in space has led scientists at Los Alamos to satellite exploration of the magnetosphere and of a wide variety of other space phenomena. Some of the instruments aboard such spacecraft have been designed to measure the interplanetary medium and planetary bow shocks, and we are doing theoretical studies in support of these observations. A related study is our work on cosmic particle acceleration. The information about energization of particles at interplanetary shocks may have applicability to shocks of much more cosmic proportions, such as those presumed to exist in supernova remnants.

We are also exploring the interplay between the solar wind [the hot, expanding corona of the sun] and the magnetic field of the earth. This interplay produces the magnetic structure we call the magnetosphere, the tenuous plasma region that makes up the uppermost part of the earth's atmosphere. We are doing computer modeling of the entire magnetosphere and, furthermore, are developing computer network links to many other institutions involved in similar work.

In a more practical vein we are using our advancing technology to do experiments in which we release chemical tracers into the ionsphere or even deeper into the magnetosphere to learn in what way these additives may modify the outer parts of the earth's environment.

Still another project is attempting to use an existing satellite in a different and innovative way. The International Sun-Earth Explorer 3 [ISEE-3] spacecraft has been orbiting at the L-1

LANDT: Certainly the Laboratory is aware of its obligation to help the country defend itself and to maintain a balance of technologies. Right now I am assigned to the Weapons Advanced Concepts Program Office, which was begun a year ago to try in a practical way to determine which technologies really make a difference for the national defense so that the country won't throw its money away on the wrong things. The Laboratory management is very interested in addressing this issue, and they have put dollars behind it and people to work on it.

ROCKWOOD: Today the government's method of doing business is very much applied and mission-oriented. Although basic research is also essential to our national security mission, it is often overlooked, and the national laboratories are handcuffed in this area by administrative limitations. People here have to be clever in extracting from their mission-oriented programs good basic results in science. I think Los Alamos has been rather successful at that.

WHEATLEY: Do you think mission orientation is a good thing? As a matter of principle?

ROCKWOOD: Moderation in all things.

BAKER: I think we must fight this trend toward applied work only, toward everything having an immediate payoff. A national laboratory should play as active a role in basic research as any laboratory. The country will suffer in the long run if we don't.

ROCKWOOD: Often the most exciting and fundamentally useful part of a program is not its stated objective but some unplanned spin-off. In the laser isotope separation program, spectroscopists working to explain the spectrum of the octahedral molecule UF_6 discovered that the octahedral symmetry group had originally been analyzed incorrectly and had been wrong in the literature for years. Even a very applied program may yield results of use to basic science.

BAKER: That's certainly been true in space physics. The Vela satellite program to detect nuclear explosions deep in space was a mission-oriented project, and we continue to have test and verification activities. To accomplish that practical goal we had to place instrumentation on the spacecraft to measure the environment. As a result, many properties of the magnetosphere were discovered.

Now the space physics groups are involved in a number of activities on collisionless shock waves, cosmic particle acceleration, the interplay between the solar wind and the earth's magnetic field, and the exploration by the International Sun-Earth Explorer 3 satellite of the night side of the earth.

SCIENCE: *How do you get funds for all these activities?*

BAKER: In a variety of ways. We have been able to obtain reimbursable funding from NASA [National Aeronautics and Space Administration] for some of our projects. But the continuing money from the weapons program gives us more stability than we could ever obtain from reimbursable funding alone. When we get our funding from the DOE [Department of Energy] or from the Laboratory, we are better able to make long-range plans. It's fortunate for us that the Europeans are also participating in many of our scientific satellite programs because the European Space Agency plans much further ahead than NASA does.

HYMAN: There are some problems with diversified funding. The Mathematical Modeling and Analysis Group in the Theoretical Division is almost completely basic research, and we also have been obtaining some support from outside the Laboratory. The largest block grant we have supports only one and one-half staff members. Because our funding comes in such little pieces, we are perpetual job hunters and odd jobbers—always knocking on a different door.

ROCKWOOD: The country hasn't learned how to fund basic science at all. Research doesn't integrate with time. Each administration point on the sunward side of the earth for about four years. The L-1 point, the sun-earth Lagrangian point, can be thought of as an imaginary center of mass around which the satellite has been traveling in a large looping orbit. Now this satellite has been moved into the earth's distant magnetotail and is orbiting well downstream on the night side of the earth. It will be the first spacecraft to explore that region in space. To accomplish the move, the satellite's gas-jet thruster, which ordinarily performs minor station-keeping orbital adjustments, was used to move the craft in such a way that it encountered the moon's gravitational pull and got a lunar gravitational assist to kick it deep into the magnetotail. It is not in a stationary orbit, and thus the lunar encounters must occur every one to three months in order to keep the satellite deep in the magnetotail. Eventually another lunar push will occur, and ISEE-3 will go on to intercept a comet. This will be the first time that any spacecraft has gotten close to a cometary body.

Bob Farquhar, a very creative guy at NASA who seemingly can move any satellite anywhere you want using any other celestial object, helped with the ISEE-3 project and has also helped to plan what is called the International Solar-Polar Mission. Because we don't have enough energy in most launch vehicles to get significantly out of the ecliptic plane [the plane of the earth's orbit], we are sending a satellite out to Jupiter to get a large gravitational kick from that massive planet. The spacecraft will then move above the ecliptic plane and travel high over the sun's pole, another previously unexplored region. ■

comes in and has a new policy. Basic science suffers more from these oscillations than it would from a low level of sustained funding. And I believe Los Alamos suffers more from funding oscillations and changes in direction than other national laboratories. Our normal attrition rate is about 4 per cent per year. Any change in direction by more than that amount involves moving people around. People's skills are not always totally applicable to a different program, and those who are not absorbed by other parts of the Laboratory are not absorbed by the town at all. It is this very closed environment, which drastically constrains our flexibility, that I see as a major problem for the Laboratory. It always has been so.

Returning to the question of the funding of basic research, I feel that, although the government can't just pour out money and expect nothing in return except good intentions, the funding "pendulum" has swung too far toward applied activities.

WHEATLEY: Some of you would say that Los Alamos ought as a matter of principle to devote some fraction of its work to purely unqualified basic science, the sole motive being to understand things better and to develop knowledge or whatever—to have fun, really. I would like to suggest that perhaps that's not true. Perhaps it is our responsibility to articulate the possible relationship between our work and some appropriate mission of this Laboratory. I am not thinking of explicit applications, necessarily. Let me give you a personal example. I think that it is appropriate that my work in thermal and condensed-matter physics should feed into thermal technology, broadly defined, that is to say, into technologies that involve the concepts of energy, work, heat, temperature, and so on.

Right now I am working on heat engines. I had set myself a semipractical problem that no one in industry would define as practical of course—but it was. It had to do with producing cold very simply. I had an idea for doing that with acoustics, so I started playing around with the idea, developing it, and soon—meaning one year later—I found that what I was doing seemed to me to have very broad implications. Now I have put possible applications off to one side, and I am looking strictly at the basic science, at the fundamentals of it. I think I have identified what I regard as a new principle applying to heat engines in a very general sense. I do feel a responsibility ultimately to be able to draw a connection between the basic scientific work I do and some technology.

KOLB: I don't feel that way at all. There is a real necessity for nonmission. For fifteen years people have been looking at magnetic monopoles, intensively, just for pleasure, and for the past five or six years have been studying grand unified gauge theories—same motivation. Recently, Rubakov in Russia and Callan at Princeton have proposed that monopoles can catalyze proton decay, can just completely convert the rest mass of protons into energy. It will be another five years before it's worked out. Now something like that would have a tremendous payoff, would be comparable to Otto Hahn's discovery of fission. But it never could happen in a mission-oriented environment. No one told these people they should study monopole structure because it might have important applications. And no government agency has told me I should be studying them, either.

WHEATLEY: I'm not waiting to be told what I should do, either. For instance, I would feel perfectly fine studying spin-polarized hydrogen, a project in which I am very interested. Nor can I tell you what gadget that might be used in, but I do see that it is part of the foundation for thermal physics and that we ought to understand it.

KOLB: I don't choose research projects by wondering if they will have any impact on technology.

BAKER: Aren't you thinking of beam weapons systems using monopoles?

KOLB: If I think about it, it is only after doing the basic science.

HOWE: Is it necessarily the basic researcher's responsibility to come up with the utility of it? There are, perhaps, other people who are more interested in the engineering side, so they take the proton-monopole catalysis concept that Rocky mentioned and say, "Well, let's develop starship drives; let's design power reactors!"

SCIENCE: *Rocky, how* do *you choose your research projects? You've said how you don't choose them.*

KOLB: I don't know, actually. I don't know what I am going to be doing tomorrow or when I go back to my office. I read the literature and see what other people are doing. This communication is very important. I follow the direction the work is going.

HYMAN: You may recognize a problem as being important, but in the end the choice is subjective. A question gets under your skin, and you can't let loose until you understand it. That's the driving force behind science—the need to understand. As far as Rocky's responsibility to the Laboratory, that has become clear as he's talked. His obligation is to push back the frontiers of basic science—that's his job description. At the same time every scientist has a responsibility to the overall health of the Lab. Whenever you discover something that could be applied in a programmatic effort, you go down the hall, knock on doors, and make sure the right people know about what you have done.

KOLB: When I first read about Callan and Rubakov's work on monopole-catalyzed proton decay, I was at Aspen, and I said, "Well, I have to get back to Los Alamos and tell people about this," but then Stirling and I decided it couldn't work, so I didn't go knocking on doors.

WHEATLEY: Coming back to the missions of the Laboratory, I understand why we should be doing some basic science and much fundamental technology, that is, research on problems whose ultimate objectives are fully seen. However, my own view concerning applied work and hardware is that if you have a particular, well-defined job to do, the private sector would probably do it better.

HECKER: I would disagree, John. The weapons mission is a specific job, and we have done it very well.

WHEATLEY: The weapons case is rather special because of the national security problem. Suppose that you took the secrecy requirements away.

BAKER: In fact, private industry does secret work, builds all the components. We provide the overall science and technology. I don't think secrecy is the defining factor. The national laboratories are most effective doing both the theory and the design development of jobs that are high risk and from which an industry couldn't expect a profit in a short term. Fusion is another example.

HYMAN: Our exceptional facilities also give us an edge over industry. The two thousand scientists at Los Alamos comprise a pool

Sig Hecker on Materials Science

Materials are the *sine qua non* for new technology. At Los Alamos we have been in the business of processing new materials for technological needs from the very beginning. Now materials processing is becoming more sophisticated as we learn to exploit our understanding of materials on an atomic level. Our work on rapid solidification and ceramic processing exemplifies this trend.

So-called rapid-solidification-rate materials are made by cooling the liquid state very rapidly, on the order of a million degrees per second. The rapid solidification avoids equilibrium decomposition and consequently affords the opportunity to create materials with new and novel structures. For example, if you smash a liquid metal between an anvil and hammer or spin it against a cooled, rotating wheel, you can create a metallic glass, that is, an amorphous metal rather than a metal with the normal polycrystalline structure. Properties of metals depend critically on their crystal structure, or, more specifically, on the defects in the crystal structure. By creating an amorphous metal, we eliminate grain boundaries, which contain many defects and are therefore places where corrosion begins. Consequently, these metallic glasses have good corrosion resistance as well as high strength. Our rapid solidification work at Los Alamos has been applied mostly to processing actinides.

Our work in ceramics processing is aimed at a new class of structural materials for high-temperature environments, such as those involved in fuel processing and power generation. For example, a ceramic turbine might be used to achieve higher operating temperatures and higher efficiencies.

State-of-the-art work is being done in two areas: processing of dense ceramics without densification additives and growth of ceramic whiskers. The ceramics of greatest interest to us, silicon carbide and silicon nitride, must be made at relatively low temperatures to avoid decomposition. A densification additive forms a glass phase between the powder particles and essentially glues the particles together. Unfortunately, during high-temperature service, in a turbine for example, the glue turns glassy and the ceramic loses strength. To eliminate the need for an additive, we have developed a technique for making an extremely fine, extremely reactive powder that shows great promise of densifying at low temperatures. We form the fine powder particles, which have diameters on the order of hundreds of angstroms, by a plasma-assisted chemical vapor-deposition process. In this process the constituents, such as silicon and carbon, are carried by appropriate gases and are reacted in a hot argon plasma. We are also using the Laboratory's expertise in shock loading to activate ceramic powder containing larger diameter particles. The idea is to produce a large concentration of defects on the surface of the particles before attempting to consolidate them.

Ceramic whiskers, a field in which we are the world leader, are long, single-crystal fibers of, for example, silicon carbide or silicon nitride, with diameters that vary from less than a micron to maybe ten microns. These single crystals are grown by a process called the vapor-liquid-solid process. They are essentially defect free and have enormous strengths, from ten to fifty times that of structural steel. We are now trying to incorporate the whiskers into a composite material—a glass matrix, a ceramic matrix, or a glass-ceramic composite—to make high-temperature materials. Essentially, we are using processing science to control the strength and the ductility of materials on a microstructural level.

Another area that is not new, but extremely fascinating, is the actinides. In the last few years a marriage of condensed-matter physics, chemistry, and metallurgy has helped us to understand the intriguing electronic and magnetic properties of these elements and, in particular, how they determine the macroscopic properties of plutonium, uranium, and americium. For plutonium, especially, the only way to understand it is to understand the role of its bonding f electrons. For example, because the f-electron wave functions possess odd symmetry, bonding of these electrons favors unusual crystal structures with low symmetry. People in academic circles are now becoming very interested in the actinides because they offer new physics. ■

of knowledge found in only a very few places. Also we have five Crays and a complete set of shops.

WHEATLEY: We do have a complete set of shops, but it costs fifty-five dollars an hour to use them.

HYMAN: But they are at our disposal.

COLGATE: Just for a moment let me reduce the main missions and the main capability of this Laboratory to plain terms. Suppose we didn't have a Laboratory. Why would Congress, the politicians, want to start one? The only reason would be because they were scared: scared of losing the country—that's our national security mission—or scared of losing our way of life and our power—that's the energy mission. Fear for the future motivates the existence of this Laboratory. Politicians would never fund science from purely altruistic motives, and purely educational business would be in the universities where it belongs. But how do you make sure that a new idea doesn't come up to bite you from the rear, as Sputnik did? You have the most brilliant people around to think up all the new ideas that are possible before someone else thinks of them. So the basic capability of this Laboratory is its brilliant individual scientists. If someone wants you to come to the Laboratory, why do you accept? Because people here are doing the most exciting research in your field, and because you believe in your own ability.

ROCKWOOD: There's something I worry about, and I'd like to mention it here. At moments of international crisis, programs for the national laboratories are easily defined. But during periods of uncertainty about the future, and especially during periods of economic stress, the selection of programs is not so simply made. One of the strengths of Los Alamos internally is its great freedom of thought—freedom to disagree, to discuss openly with management the pros and cons of particular technical endeavors. It makes us stronger to have had these discussions and to look at all sides of a problem before going into it. But we should speak with only one voice to the external world. We don't need two, three, half a dozen people showing up in the same office in Washington, each with a different opinion as to which major programs the Laboratory should be pursuing.

SCIENCE: *While you more or less agree that the development of high technology for national security is the Los Alamos mission, the specific emphases and manner of carrying it out remain open to discussion. Perhaps we should turn now to some of the specific areas of research and development that are clearly important. Carson Mark has commented that many of the problems in technology development are materials problems. Sig, would you tell us what is being done at Los Alamos in this area?*

HECKER: Our materials science effort demonstrates the exciting and productive relationship that exists between theory and experiment. It is one of the beauties of this Laboratory that metallurgists, physicists, and chemists work side by side. Our main interest in materials processing, without question, has always come from the weapons program. Weapons designers, be they physicists or engineers, come to us with requests that to them seem exceedingly simple and to us almost impossible, at least at first glance. For example, the physicists wouldn't hestitate to ask us for structural air, that is, something with no density but enormous strength. Faced with sophisticated problems for years and years, we've learned how to tailor-make many special materials.

We have also done some basic research in materials science, and in the past few years we have begun to apply our understanding of materials on an atomic level to materials processing. One example is rapid-solidification-rate technology to make amorphous metals with high strength and good corrosion resistance. Another is ceramics processing; we are attempting to make materials for high-temperature environments, such as composites containing single-crystal ceramic whiskers.

LANDT: Electronics is another field that combines ideas and applications; it's partly software and partly hardware, and it's a crucial part of future technologies. I would like to put before you a statement by Dr. DeLauer, Undersecretary for the Department of Defense. Dr. DeLauer insists that electronics is the most critical of all technologies for the maintenance of peace, and he claims that "Further development of the electronics technology base of the United States is as important to defense today as the atomic bomb in World War II."* I think it's time the Laboratory took its electronics seriously.

BAKER: There are, however, a lot of good electronics firms.

LANDT: We are working on several projects that could make significant contributions in electronics—areas that private industry is not touching. These include high-speed electro-optic switches and thermionic integrated circuits that have important military as well as commerical potential. We are also developing high-power microwaves from lasers. This is research that could not be done without the exceptional computer and experimental facilities at Los Alamos.

SCIENCE: *Since we have mentioned speaking freely, I'd like to ask Steven whether there's anything he can tell us about weapons design work.*

HOWE: Most of what we do is classified, but I can say that we work to get better codes, better computational abilities to describe the processes in the weapon, to put in the things we do know so that the things we have to extrapolate can be better estimated. In the year I have been here we have come up with several interesting pursuits. One is in low-energy nuclear physics: there is a process that we think exists in the weapon but that we don't account for in the codes. This particular development is interesting because we have shared it with Livermore, and we have collaborated with them in getting it into the codes and making estimates. We also do secondary design work on weapons materials, attempting to understand basic processes. Generally we aim to satisfy the military requests and to come up with smaller, more efficient devices. We are continually looking at new

Jeremy Landt on Electronics

Our heavy reliance on the world of electronics has led Los Alamos into several fledgling projects that show great promise for the future. One is the development of the high-speed electro-optic switch, which can be used to probe integrated circuits with pulse widths of 50 picoseconds or less. Understanding of semiconductor physics on these short time frames is essential for development of reliable, very high-speed integrated circuits for future weapons systems. The first generation of very high-speed integrated circuits is largely based on extrapolations of existing technology. To go beyond will require new technologies and understanding that industry does not have at present.

Another device under development is the thermionic integrated circuit, which is inherently hardened to radiation and EMP phenomena. Before research on this device began at Los Alamos, an attempt to commercialize the technology failed because the basic physics was not understood. We could use this device to instrument nuclear and geothermal systems, as well as in military applications.

The area I find most exciting, however, is the broad area of high-power microwaves. We are working on novel generation mechanisms as well as novel applications. One new generation scheme involves the Helios laser, the Laboratory's high-power carbon dioxide laser. Large numbers of hot electrons are generated in high-power laser targets. A carbon dioxide laser produces far more hot electrons than do lasers operating at shorter wavelengths. We are presently investigating ways of converting these electrons to high-power microwaves. The power levels achieved to date are very impressive and probably can be improved much more. At present this research cannot be done anywhere else in the world. Los Alamos has both the computer codes to handle the flow of particles in electromagnetic fields and the experimental facilities to benchmark the codes. ■

**Richard D. DeLauer, "The Force Multiplier,"* IEEE Spectrum, *October 1982, p. 37.*

Brian Crawford on Life Sciences

Several exciting things are happening in life sciences. We are using laser-based flow cytometric methods to separate chromosomes from mammalian, including human, genomes. DNA from these isolated chromosomes can be cloned by recombinant DNA methods, allowing studies of the basic structure and functional organization of the chromosome. Los Alamos is one of perhaps three labs with the requisite expertise in biophysics and molecular biology to perform this work, and recent NIH [National Institutes of Health] funding to establish a Flow Cytometry National Resource is fostering progress in this area.

We are also working on cellular oncogenes. These genes are thought to control the evolution of the normal cell toward malignant change. The isolation, that is, the cloning, of such genes by recombinant DNA methods and the reinsertion of these genes into normal cells, by a process known as DNA-mediated gene transfer, permit us to study how specific oncogene expression can result in cancerous change. We are also studying the role that gene rearrangement, which can result for example from chromosome damage, can play in the initiation and progression of cancer. This work relates to DOE concerns regarding the effects of both ionizing radiation and the by-products of fossil-fuel development and consumption.

Another exciting development is the establishment of an NIH-funded DNA sequence database in the Theoretical Division. Sequencing, or decoding, of the genetic code in cloned fragments of DNA is meaningful only if such information can be stored, retrieved readily, and analyzed. Just consider for a moment that each mammalian organism expresses on the order of fifteen thousand distinct genes in a cell—not to mention that each cell has DNA encoding for an amount of unexpressed information that is several orders of magnitude greater. Software development for the analysis of the stored sequences will be pursued concomitantly with this Herculean bookkeeping effort. ■

things and attempting to improve the codes both in X Division where we do theoretical weapons design and in T [Theoretical] Division. We do interesting work, and I find it kind of sad that we can't tell everybody about it. Clearly we could do better if we could talk to people.

BAKER: Do you find it difficult to get rewards from your work because you can't talk with more people about what you do, can't publish results?

HOWE: In some sense your ideas are rewards in themselves. If they work, you know you have made a gain, perhaps even contributed to unclassified scientific efforts like inertial confinement fusion, which is also being studied in our division.

SCIENCE: *Is it difficult to pick up information you need because your problems are classified?*

HYMAN: I really think it is. It is frustrating on all sides not to be able to express an interesting scientific question in the context where it arises. You notice the difference at national physics meetings between the typical scientist and those working only on classified problems. The ones working on unclassified problems can go to the blackboard and describe everything in minute detail, get immediate feedback, and also know that people will go home and continue thinking about the problem. When people first come to X and T Divisions, they continue to go to physics conventions as they did before. But if they work only on classified problems, often within the first few years their attendance drops off very fast. Some just stop attending national meetings and interacting with the outside world.

At the Center for Nonlinear Studies [CNLS] we are trying to encourage interactions between the classified and unclassified research areas by organizing mixed workshops. In these workshops the first two or three days are unclassified and uncleared university scientists are encouraged to attend and speak. On the last day classified questions related to national security are addressed, and the attendance is limited. The last such conference was a joint X-Division/CNLS workshop in February on interface instabilities.

A problem we have not been able to overcome is that numerical results generated by a classified code are classified—even when the physics model, the data tables, and the numerics used in the run are unclassified. This restriction greatly inhibits interactions with computational physicists outside the Laboratory.

SCIENCE: *How open is the communication between T and X divisions?*

HOWE: We rely heavily on our communication with T Division people.

HYMAN: Mostly it's between people you've worked with for years or know from the coffee machine. And the interchange is more limited now that the two divisions have been physically separated. We are trying to get more joint seminars so that we can indeed hear what people doing unclassified research learn in the outside world and then relate it to our needs.

HECKER: It is a poor substitute to have to depend on T Division for your information.

HOWE: It doesn't really work.

SCIENCE: *Is an effort being made to change the situation?*

HYMAN: Yes, there's been a change in the attitude of management.

In T Division we've always been very strongly encouraged to publish at least one paper, if not more, a year and to present at least one, if not more, at a national meeting. Some of the same emphasis is now appearing in X Division.

HOWE: We are getting more new people straight out of universities, and I think those who are new are interested in the national meetings. Getting back to our relationship with T Division, I would like to see us, as designers, integrate better with the work in T Division. For example, we really don't have a well-defined effort to do nuclear physics type research in the weapons physics business. We do our job for the military. They say, "We want this beast," and so we take what the codes can give us, and we design the creature. The T Division staff doesn't have this limitation and their work in nuclear physics is relevant to what we do in weapons.

BAKER: I know that some people in X Division work enthusiastically with the space groups. They have a number of large computer codes that they like to test on a variety of systems to see just how well the codes predict behavior. The magnetosphere is a large plasma system with magnetic fields; they like to try to model that. We do such modeling, too, and like to compare the results of our different codes.

SCIENCE: *I want to ask you about the young people in the weapons program. Are they there because the problems are interesting or because they have some feeling of commitment to the development of new weapons?*

HOWE: Many of them are in there because they did their theses in areas used in weapons research. Weapons development is such a multidisciplinary field; everything in the world is involved in making this thing go. Chemistry, physics, nuclear engineering, hydrodynamics—almost any field you name is involved. I would say people's motivations vary.

HYMAN: Many people have come into the weapons field because at one time they recognized that controlling fusion is one of the most important unsolved physics problems of the century. Much of the knowledge and data needed to crack the controlled fusion problem is classified. Once in the system, people find the weapons-related problems equally or even more fascinating and rewarding.

HECKER: Because of the strictures of classification, people rarely choose to come to the Laboratory to do materials research for the weapons program. People come here to do research in other areas and then wind up working on weapons problems because they are so interesting. We do have a corps of extremely dedicated people who build prototype hardware, develop our local shots, and design Nevada Test Site shots. But the tight ring of security really stops the flow of ideas from the outside in. Our metallurgists working on plutonium have been so strictly limited that we have tried to give them a cross section of other work, but an enormous amount of materials expertise remains outside the reach of the Laboratory.

SCIENCE: *A new Center for Materials Science has been created at the Laboratory, as well as the Center for Nonlinear Studies and a branch of the Institute for Geophysics and Planetary Physics. Are these centers aimed at alleviating the communications problem?*

HECKER: Yes. Don Kerr has recognized the overall problem. The Center for Materials Science has brought us in close contact with first-class materials science people outside the Laboratory.

HYMAN: The Center for Nonlinear Studies has had a similar impact. We sponsored over three hundred visitors last year. Besides the week-long conference each year, we have a number of workshops in areas we've chosen to target. One target this year is understanding the creation, stability, and evolution of patterns, fronts, and interfaces. There will also be workshops on cellular automata, implicit methods of differential equations, fracture mechanics, science underground, synthetic metals, and biopolymers. And what is even better than solving immediate problems is bringing together from the Laboratory, industry, and universities people on a one-to-one basis—establishing relationships that can continue for many, many years.

BAKER: In contrast, the Institute for Geophysics and Planetary Physics is directed toward interactions with professors and their students. We are a resource of the University of California in particular, and we now have a number of their graduate students working here for a year or two.

KOLB: This type of interaction not only helps us; it brings in people who then discover what is going on in the Laboratory. Half the people taking part in this discussion had their first contact with the Laboratory either as graduate students or as postdocs. Both the graduate student program and the postdoc program are really excellent ways for the Laboratory to recruit good people. I strongly believe it would be to the long-term benefit of the Laboratory to enlarge these programs and the visitor program as well.

ROCKWOOD: We should also work closely with the universities to make both students and faculty aware of the directions in applied science and the particular types of people that we see we are going to need. We can give universities access to such facilities as LAMPF, Antares, and Helios as research laboratories for their students; in return they may become more familiar with this Laboratory and be more responsive to our future needs.

HYMAN: In line with this thinking I should point out that the Graduate Research Assistant program is probably the most effective and least expensive of all of our advertising. But it's under-utilized, and I'd like to see it used more.

CRAWFORD: The closer our contact with graduate students, the better off we are, I think. It's a way of advertising the incredible potential and diversity of this place—some of it realized and some still untouched. It's difficult to overstate the importance of the Laboratory's diverse capabilities. I think there's a real need to keep

Laboratory Support for Basic Research

The Laboratory has always recognized the need to support a wide variety of basic research, and for most of the Laboratory's history, that research was funded entirely by the weapons program. During the 1970s, however, budgetary constraints made it increasingly difficult to maintain the level of so-called Weapons Supporting Research, and in 1975 concern about its steady decrease prompted Harold Agnew to found the New Research Initiatives program as a supplement. However, despite the Laboratory's growth and widened spectrum of activities, Weapons Supporting Research funds continued to be the dominant means of Laboratory support for basic research.

In fiscal year 1982 Donald Kerr combined and expanded the Weapons Supporting Research and New Research Initiatives programs with establishment of the Institutional Supporting Research and Development program. This new program incorporated the following principles, many of which required new and extensive plans on the part of everyone involved.

- The program should be Laboratory wide and should include a broad spectrum of research and development related to all Laboratory programs.
- Projects should be consistent with and in support of the Laboratory's basic missions.
- Funds should be distributed according to a fair scheme that encourages competitive proposals and ensures optimum investment of resources.
- Support should be derived proportionately from all Laboratory programs.
- Accountability of funds should be reasonable and consistent with normal practice.

The ISRD program has definitely improved the manner in which discretionary research funds are allocated and the status of funded projects is reviewed. Considerable freedom is exercised by the Laboratory's associate directors in organizing and evaluating projects under their directorates. As is usual with any new program, some shortcomings have been recognized and some evolution is expected. It is evident that, in spite of the healthy challenge of submitting competitive proposals, there have been too many proposals and they have, for the most part, been too long. Paperwork is being reduced, and a system of triennial, rather than annual, review is being developed for some projects.

The accompanying table lists the distribution of ISRD funds among various broad categories in fiscal year 1982.

DISTRIBUTION OF ISRD FUNDS IN FISCAL YEAR 1982

Research Category	Allocated Percentage of Total Funds
Materials Science and Chemistry	32%
Program Development and Applied Technology (Energy and Defense)	25%
Mathematics, Techniques, and Computer Modeling	13%
Nuclear Physics and Nuclear Chemistry	11%
Medium- and High-Energy Physics	8%
Plasma Physics and Astrophysics	4%
Earth and Space Sciences	4%
Life Sciences	3%

not just students, but the whole country, informed about what we're doing and can do. One important example in life science research is the new DNA sequence data base being established in the Theoretical Division and funded by the National Institutes of Health. This will be a comprehensive computer-based library of DNA sequences designed specifically as a resource for scientists around the world who are doing recombinant DNA research. Eventually we may be able to produce a computer-based, electronic journal that bypasses conventional publication. Scientists could submit their DNA sequence data for review and receive results in recombinant DNA research electronically.

SCIENCE: *How do new projects such as the DNA sequence library get started?*

HOWE: First someone has to have an idea and that usually happens quite informally. We sit around and talk and suddenly some guy comes up with a neat idea.

COLGATE: That's right. Some of us don't know one another very intimately, but sooner or later we will meet. I will bump into John and start talking about cryogenic systems for fractional charge separation using superfluid liquid helium as a charge separation drift chamber.

ROCKWOOD: Once the idea is hatched, you might try it out with what is called bootlegging. You do the experiment or the calculations at your own discretion, but generally with the knowledge of the group leader, division leader, or whoever else is involved. If the idea shows real promise you may be funded through Institutional Supporting Research and Development [ISRD] money. This is the Laboratory's discretionary fund. It has traditionally been used for basic research, but more recently it has also been used to fund new applied programs. I, for one, believe the applied programs should receive an equal share of this money.This is our investment in the programs of the future, and, in the final analysis, only programs pay the Laboratory's bills.

HECKER: The fact that this Laboratory has the foresight to take a meaningful fraction of its total income and plow it back as discretionary research is fantastic. At many other places the discretionary research money is more like one per cent. We do have an enormous opportunity for internal research. Of course, there has been a lot of upheaval recently about having to write proposals every year for ISRD money.

COLGATE: I think proposals are a darn good idea. I never did have to do them at Livermore. Then at the university I ended up having to write twelve a year. They are never easy, but they are really worth it.

BAKER: They do help people who didn't know what they were doing to think about their work a little more, but on the other side of that coin I think management can really be an obstacle.

COLGATE: Yes, if proposals are not reviewed correctly, you end up with a mess. Most proposals are now judged by the Laboratory management and the Senior Fellows, but this does not always constitute peer review.

HECKER: I agree that we do need more accountability than we used to have. However, one simply cannot set up an environment to do good basic research if proposals are required on a yearly basis. Also, the people making the decisions have become farther and farther away from the people who really know what is going on. I'd like the authority and the responsibility for research programs to rest with the divisions. By all means have an advisory panel of outside peer experts to judge the quality of the research, and if the results aren't good, then fire the division management.

BAKER: I've found that the handing out of Institutional Supporting Research money is based too much on historical factors rather than on quality of research. There is no competition in the true sense, that is, based on demonstrated scientific competence.

HECKER: That problem has been addressed to some extent. Two years ago six working groups were set up to look at areas that were not well represented traditionally, and I know that materials science has been receiving more support recently.

COLGATE: Perhaps the ultimate mechanism is, once again, the individuals. To my mind the Lab is put together of people who have an absurd sense of ego; that is, they have the drive and the motivation to back their own original ideas.

HYMAN: It's true that most projects have started with individuals who were aware that something was about ready to break. They went out and wrote proposals; they got up on their soapboxes; they sold their ideas and started small. Sometimes the ideas fizzled out, but other times they turned into whole divisions.

Rocky Kolb on Cosmology

"Cosmos" is the Greek word meaning order, and the basic goal of cosmology is to understand the universe on the basis of physical law. By applying physics to what we see in the universe, we endeavor to understand the structure of galaxies and the origin and large-scale structure of the universe.

Within the past five years or so some very interesting and very bold particle physics theories have been hypothesized. They model the physics of incredibly small scales—down to Planck's scale, which is about 10^{-33} centimeter. These theories are extrapolations, but there is some physical basis to them and they imply certain things about the universe. For example, they predict proton decay and the existence of magnetic monopoles. If these predictions are correct, then we now have models of the structure of matter under unbelievably extreme conditions of density and temperature, and we are in a position to study the very, very early universe. By the early universe we used to mean 1 minute or 1 second after the big bang. Now we can talk about 10^{-33} or 10^{-38} or 10^{-40} second because we believe we have a model of the underlying physics with which to do the astrophysics and cosmology.

Some practical questions we might answer are how many magnetic monopoles are expected to be around, what are their properties, and how would one look for them. Another possible insight is understanding the asymmetry of the universe in baryons—that is, why there aren't an equal number of baryons and antibaryons. Unfortunately the big bang is not an experiment that you would want to—or could—duplicate.

Study of the early universe leaves an interesting unanswered question: why the universe is so old. If you look at the Einstein equations that describe the evolution of the universe, the only

BAKER: Jerry, what reception do you find to suggestions being made by the Weapons Advanced Concepts people?

LANDT: Very good in general, but there are some people who resist change and don't like to see things at the Laboratory change.

HOWE: I find in the weapons program that you can have a wonderful idea either in software or in hardware, and, fine, they will help you develop it and make the best calculations possible. But then they fail to implement it. Furthermore, we are being urged to develop our own codes rather than just to borrow from Livermore. And in fact we do have several new ones, but I find there is some resistance to changing several hundred thousand lines of a code and putting in the new stuff. The same kind of reluctance appears in the hardware; it takes several years to get a materials idea implemented.

KOLB: Is that a management problem?

time scale that appears is the Planck time, which is about 10^{-45} second. It is rather hard to understand why today, ten billion years, or 10^{60} Planck times, after the big bang, the universe hasn't either recollapsed or expanded to an extent that the gravitational attraction of the matter is irrelevant in the expansion. Today we cannot determine whether the universe will expand forever or eventually recontract, since the kinetic energy of expansion is almost equal and opposite to the gravitational potential energy. This seems to imply that in the initial expansion the kinetic energy balanced the gravitational energy to something like one part in 10^{56}—essentially a zero-energy system. This conundrum has a possible explanation if the universe underwent a strong first-order phase transition. An active field now is phase transitions in the early universe. This is a true interdisciplinary field, bringing in particle physics, general relativity, and statistical mechanics.

Our investigations may also have a number of reciprocal implications for particle physics. It has become fashionable every time a particle physics model is proposed to look for the astrophysical impact of it. You try to see whether the new model does things to the universe that you can't allow. For example, does it lead to too much mass density in the universe? Another example is monopole-catalyzed proton decay. Colgate and I have pointed out that such decay would have a terrible environmental impact on neutron stars. The work we have done leads us to believe that either monopoles do not catalyze proton decay or that monopoles don't exist, which would really be a shame because their existence would have enormous practical implications. ■

HYMAN: It is somewhat a management problem in that the codes have been allowed to grow unstructured for so many years that they have become the unmanageable things they are.

HOWE: It may be an external problem—one caused by whoever is using the weapons.

CRAWFORD: The external response to new ideas probably varies greatly from agency to agency. The Office of Health and Environmental Research, which oversees much of the research in the Life Sciences Division, is quite receptive to new programs.

COLGATE: Other offices of the DOE are also receptive. For example, Rocky has had ISRD support for some time doing far-out research in cosmology relating to conditions in the early universe. But what's really relevant is that last year the Office of High Energy and Nuclear Physics saw fit to pick up part of his funding. Nothing ventured, nothing gained!

SCIENCE: *With regard to external support for new ideas, the Laboratory is encouraging more interactions with industry. How will this affect the Laboratory?*

ROCKWOOD: I would say that a closer union of this Lab and industry would be mutually beneficial. The best single thing that has happened is that the DOE may now allow patent rights to remain with a funding company. Private industry can now put some money into a national lab without losing all rights to patents that emerge from the work. For instance, an industrial organization that wants to get involved in a new venture requiring a group of plasma physicists wouldn't have to hire twenty of their own while they got started. Instead, they could hire our expertise in that area to help them get started—a healthy collaboration.

WHEATLEY: I really think that is right.

ROCKWOOD: I see us starting to make some progress. We have money coming from Westinghouse to help look for a method of enriching certain isotopes that they are interested in as a company. They would have refused to invest this money in us a year ago.

BAKER: The hot dry rock project is a related example. Money is coming from a variety of sources, such as the Japanese government and the German government, as well as our own government.

SCIENCE: *We hire the people and they fund them?*

ROCKWOOD: They hire our people, if you will. They contract to us to do a specific task that saves industry from building up a highly specialized group of people they don't need for the long term.

HYMAN: The kind of basic research a lot of us do is oriented toward the very large problem with very limited applications. Take the supercomputers. There just aren't that many supercomputers out there. Most vendors can't afford to support the effort needed to develop new algorithms and software that push these computers to their limits. Yet it is quite appropriate for us to do that here.

HOWE: I can forsee that industry funding might compete with basic research for a person's time. Since it is near-term support, you are going to have managers saying, "All right, we want you guys to work on this project for Westinghouse, and you have to put aside your basic research for now."

ROCKWOOD: I think rather that industry will be wanting to use basic research that we have already completed. But I won't say that conflicts will never arise. They'll have to be worked out.

CRAWFORD: If we become closely allied with both universities and private industry, perhaps we will be able to function more as a research and development organization—taking ideas from university programs and assigning teams of researchers well qualified to test the feasibility of such ideas—with the goal of technology transfer to private industry.

SCIENCE: *Gentlemen, it seems that our relationship with industry may undergo a change. What other changes would you like to see* *happen in the future? I know I'd like to hear about the proposal for an underground laboratory.*

KOLB: Los Alamos has a proposal to build such a laboratory at the Nevada Test Site. It would be operated as a user facility, like LAMPF, and would make possible an entire class of very sensitive elementary particle experiments that require shielding from the normal above-ground radiation levels.

Los Alamos is a good laboratory for this facility because, first of all, we have strong groups in theoretical particle physics and in astrophysics. The interdisciplinary work of the facility would require a broad base in many areas of physics. We would aim to learn about neutrino oscillation and determine neutrino masses, topics that would have a large impact on our understanding of galaxy formation. We would have a chance to detect proton decay, which would go a long way toward telling us how much we understand about the origin of baryon symmetry. We could also learn many things about cosmic-ray physics and the large-scale structure of the universe. And a facility like that would generate technology in building detectors and in doing state-of-the-art experiments.

HOWE: I would like to see us expand in the space utilization business. We have a great deal of expertise in basic physics research and materials sciences, but we don't have much of a program for utilizing space.

HECKER: At the expense of the Jet Propulsion Laboratory?

HOWE: JPL is mostly involved in planetary exploration, and NASA is doing hardware development. Perhaps Los Alamos should begin programs to utilize the shuttle, to utilize the space station if it gets built.

BAKER: Those things are being considered, but so far the effort is fragmented.

WHEATLEY: There currently is an interesting cooperative program between the Center for Nonlinear Studies and the Center for Materials Sciences, having to do with conductive polymers. Wouldn't it be good to have such a program between the Institute for Geophysics and Planetary Physics and the Center for Materials Science on materials processing problems for space? We talked with a fellow from NASA who is in charge of their program for materials processing in space. That is really interesting physics—and chemistry and metallurgy and what you would call materials science.

HOWE: That is an important point. Probably the Weapons Advanced Concepts people are looking at orbital devices, but if someone comes up with an idea for an experiment to go on the shuttle, we have no one in the Laboratory who could translate the idea into shuttle-compatible hardware, as far as I know. NASA would have to be contacted. There is no given laboratory in the country to interface with industry and provide shuttle compatibility.

CRAWFORD: I'd like to see the Materials Science and Technology and the Electronics divisions combine research in their areas with the space program to develop alloys, circuits, etc. in space stations. It would be an ideal opportunity for cooperation with the private sector, and it could foster the rebirth of the space programs. It could place us at the forefront of university-industry cooperation with national laboratories.

HOWE: I'd also like to see us involved in the defense angle. The military consults with the Laboratory on a lot of concepts now, and we should have the capability of consulting in the area of space utilization.

LANDT: Interchange takes place along a number of avenues, but there are no hard and fast rules.

BAKER: We clearly have many of our eggs in the space basket for communication and for intelligence gathering, and our reliance on space is likely to grow. It is certainly something the Laboratory is interested in.

HOWE: The Air Force recently created the Space Technology Center in Albuquerque. We could have a good interaction with that phase of the military, and it would be an ideal way for the Laboratory to get involved in the space program.

SCIENCE: *Are there any other similar areas? How about computer science in terms of the future?*

HYMAN: The way that the inside of a computer works is going to change completely in the next few years, and unless we rethink how to write programs, we won't fully exploit the potential power of the new machines. Some people saw this years ago and asked that we prepare new algorithms *before* the machines arrived. Slowly the proposals went through the Laboratory and through Washington. Now, finally, we have a viable research group in the Computing Division developing new methods for machines not yet built.

There are two similar computer projects still at the proposal stage that come to mind. The first is a CAD/CAM [computer-aided design/computer-aided manufacturing] effort to model three-dimensional surfaces on the computer with a very interactive user interface. The second project is in artificial intelligence and would have many applications within the Laboratory, from providing a reliable friendly user interface for our complex computer network to applications in nuclear safeguards.

The proposal to form an artificial intelligence group at Los Alamos surfaced about a year ago, and by now it is well polished and dog-eared at the corners. A group of about thirty of our scientists meet regularly and sponsor classes and talks from visiting and Laboratory experts.

Just how speculative do you want me to be about future scientific computing?

SCIENCE: *Go ahead, speculate.*

HYMAN: All the major physics codes at this Laboratory have many similar components. At the lowest level, they use trigonometric functions—sines, cosines, and tangents. In the early days of computing, everyone had his own favorite procedure for these elementary functions, but gradually the better ones were included in the mathematics program library. In the '60s and '70s higher level routines for solving linear systems of equations, integrating ordinary differential equations, handling one-dimensional interpolation, and other moderately complicated procedures were developed and included in the computer library. But then in the late '70s the trend slowed down and in some cases stopped. Right now we have no appreciable effort developing the next generation of mathematics support software. If such a group existed, it would be writing even higher level routines: multidimensional interpolation and differentiation programs, grid generation and adaptive mesh routines that adjust the solution algorithm to the boundary of the problem and the structure of the solution, routines to help solve large systems of sparse nonlinear equations, and routines to incorporate the boundary conditions into a discrete approximation of the physics model.

For this new software to be successful, it must be compatible with existing techniques and be simple enough that in a trial run potential users can observe tangibly better results than with existing methods. The software packages that are most readily accepted are those that behave like the existing ones—only work better.

Industries and most universities that develop new software are too far removed from the production code programmers to interact with them and obtain the essential feedback. Also, the production codes are run on the most powerful computers available and those writing the software must have access to these machines. This means that we at the national computing centers should be writing the next generation of high-level mathematics support routines to be used in our production codes. At the same time we really should be getting together more with the scientists in industry and universities who are writing mathematics software. This means having a much more active visitor program in math software development and providing easy, long-distance access to our supercomputers.

CRAWFORD: I agree that we should forge ahead in our computer work, both the hardware and the software. Our national security will depend partly on our ability to lead the supercomputer field.

HYMAN: We need a coordinated effort like Japan's. Japan already dominates in applying robotics in industry. Through its Ministry of International Trade and Industry, it has identified other projects it plans to complete by 1990. One project is a high-speed computer whose capability is at least ten times that of the Cray-1. Another is a fifth-generation computer that will implement artificial intelligence—the number of inferences per second would be a hundred to a thousand times current technology. Losing our technological edge in these areas would have serious repercussions on both our economic and our national security.

CRAWFORD: I would like to insert another note of warning. Recombinant DNA techniques are ridiculously simple to master. The United States could suffer from foreign nations or even terrorist groups employing biological or chemical weapons. Our Laboratory is an ideal place—we have both physical isolation and classified research ability—to establish a defense program against such agents. Biological and chemical agents can and will be used by those with a cause, however ill conceived. Countermeasures like specific antitoxins are within reach of our present capability. The nation should move forward in preparing these defenses.

LANDT: To close this discussion, I would like to spend a minute or two talking about future defense. Historically this Lab has developed the nuclear side, but now we should try to get people to think about the other side, the nonnuclear. There is an antinuclear movement in this country and the world. Advances in electronics are going to permit some conventional munitions to have the same military impact as nuclear ones, and we should take advantage of that. These are some of the things the Weapons Advanced Concepts people are thinking about.

ROCKWOOD: I also believe the Laboratory should be expanding into nonnuclear weapons for defense. It appears that the nuclear age has, if you will, made the world "safe" for conventional warfare. Conflicts such as the kind in Vietnam, the Falkland Islands, and the Middle East seem those most likely to occur, and the ever-increasing role of high-technology weapons in those conflicts is a matter of which we must be cognizant. We are a nation that aspires to defend itself not by massive uses of people, but as much as possible by the use of high technology—and that means us here at Los Alamos. ■

The Participants

DAN BAKER: I got my Ph.D. at the University of Iowa with Jim Van Allen in 1974 and then went to Caltech as a Research Fellow in the Physics Division. While there I collaborated over a period of a couple of years with people from Los Alamos. In 1977 I came to Los Alamos for a job interview and was impressed with the interests and abilities of the people I encountered. I decided to join a group in the Physics Division involved in high-altitude physics, where I then worked for two or three years on satellite instrumentation and data interpretation. Since October of '81 I've been Leader of the Space Plasma Physics Group in the Earth and Space Sciences Division, which, I might add, is better known simply as Heaven and Earth Division.

STIRLING COLGATE: I came to Los Alamos primarily because the then Director of the Laboratory, Harold Agnew, and the then Leader of the Theoretical Division, Peter Carruthers, persuaded me to come. I had been a staff physicist at Lawrence Livermore Laboratory for twelve years and then President of New Mexico Tech for ten years. I realized that the type of research I knew best would utilize the facilities of a major national laboratory. My work in inertial fusion continues, and the ability to do astrophysics, atmospheric research, and tectonic engineering in an environment where my advice is respected and my research work is encouraged is a privilege beyond measure. In addition, becoming recognized as a theoretical physicist after initially being an engineer in the Merchant Marine and then being an experimental physicist for many years is a very great privilege, indeed. Explosions turn me on—from firecrackers to testing nuclear bombs at Eniwetok, from using the Lab's codes to calculate supernova explosions to preventing volcanic ones. Our universe started with an explosion, is filled with explosions, and by far the most extraordinary and singular one is the explosion of intelligent life.

BRIAN CRAWFORD: I was actively recruited by the Laboratory while I was completing work for my Ph.D. at Johns Hopkins University. The Genetics Group of the Life Sciences Division needed someone to investigate the basic mechanisms by which ionizing radiation, chemicals, or other agents cause gene mutation and/or malignant transformation in cells. I had the specific skills required because my thesis had involved study of the genetic mechanisms of chemical carcinogenesis. I was encouraged to apply for one of the Laboratory's Oppenheimer Fellowships, which I received in time to begin work in the summer of 1981. Since I came, I have been applying recombinant DNA methods to research on the genetic events underlying carcinogenesis. What attracts me to this Lab are its advanced facilities and, above all, its cooperative atmosphere—theoreticians are working closely with biophysicists and biochemists in very sophisticated studies.

SIG HECKER: I grew up in Austria but moved to Cleveland when I was thirteen. Indeed, I had never been west of Toledo until I came here as a summer graduate student in 1965. My visit was brought about by a gentleman from the Laboratory's recruiting office who showed me a brochure containing lovely photos of New Mexico mountains. Once here I liked the marriage of basic science and applied technology at the Laboratory. After receiving my Ph.D. from Case Institute of Technology, now Case Western Reserve University, I returned to Los Alamos as a postdoc in 1968, attracted by the excellent funding and the chance to do basic research in metal deformation. In 1973 I came as a staff member after three years in the Physics Department of General Motors. I've worked ever since in materials science, principally in plutonium metallurgy and in actinides, although I've worked on a number of projects related to the space power and basic energy programs. Two years ago I joined the Division Office of what is now the Materials Science and Technology Division.

STEVEN HOWE: I'm another of those students who keep turning up. I started coming here as a summer student in 1975 and did that for the next two years. Then in January '78 I came to do my thesis research at the Weapons Neutron Research Facility at LAMPF. After receiving my degree from Kansas State University, I spent a year at Kernforschung Zentrum in Karlsruhe and then returned as a staff member in September '81. I'm in the Thermonuclear Applications Group in the Applied Theoretical Physics Division.

JAMES (MAC) HYMAN: I was indirectly introduced to Livermore and Los Alamos at the same time. I was interviewed for my graduate fellowship, a Hertz Fellowship, by someone from Livermore, and he asked, "What are you doing this summer?" I worked that summer at Livermore, and it was the first time I saw mathematicians and physicists working in close coordination with experimentalists. It was just great—except the temperature was 115 degrees. My boss at Livermore had been here during the war, and he said, "Where you really want to go next is Los Alamos." So I did, and it evolved into a full-time job after I got my degree from the Courant Institute. I work on numerical methods and software for large systems of differential equations, equations that model the physics experiments. It's partly physics, partly computer science, and mostly mathematics.

EDWARD (ROCKY) KOLB: I received my Ph.D. at the University of Texas in '78. I interviewed here for a postdoc position, but I went to Caltech instead. Then I came here as an Oppenheimer Fellow rather than going to a university, because here I could spend 100 per cent of my time doing research rather than teaching and sitting on committees. I was attracted by the people I would have a chance to work with. It was really the people who brought me here. I did my Ph.D. in elementary particle theory, and now I'm into cosmology and astrophysics, high-energy astrophysics. I'm in the Theoretical Astrophysics Group and I work closely with the Elementary Particles and Field Theory Group, an overlap that's possible here for someone not in a traditional discipline. At universities people seem more locked into compartments: there's one person in nuclear physics, one person in atomic physics, and so forth, and it's not easy to move into new fields. Here at Los Alamos you can move quickly into exciting fields as they open up.

JEREMY LANDT: The country in the western part of my home state of South Dakota is very much like the country here, so perhaps that was a factor in my initial attraction to Los Alamos. I came here in 1967 as a summer graduate student and liked the facilities and the people. When I completed my research work at Stanford, there weren't too many jobs available at Los Alamos in the areas I had studied—radiopropagation, electromagnetic theory, and that kind of thing. But there were at Livermore, so I spent a few very enjoyable years there. But I got tired of all the people and the hassle, and when something opened up here, I applied and came back in 1975. Except for the past year, my stint here has been spent in the Electronics Division. I have worked on electronic identification systems, EMP calculations, application of radar and other electronic techniques to mapping underground fractures for the hot dry rock project, plus a little nuclear magnetic resonance work, so I have dabbled in this and that. At present I'm working in the Weapons Advanced Concepts Program Office. We're supposed to be looking at wonderful new things; we're finding lots of wonderful old things that other people have thought of.

STEVE ROCKWOOD: After finishing my doctorate at Caltech in 1969, I went into the Air Force as my obligation to the country during the Vietnam era and spent two years at the Air Force Weapons Lab. There I got into laser activities, a field entirely different from my graduate work. I came to Los Alamos in 1972 principally because the laser programs then being started at the Laboratory and the people here were stimulating. It is an exciting area to work in. A secondary consideration would have to be the New Mexico environment. My own personal way of working has been to change fields frequently, although always within physics. I started out at the Laboratory as a theorist in T Division and then became part of the fledgling isotope separation program and was Leader of the Laser Development Group until 1980. Then I took over my present job as Deputy Associate Director for Inertial Fusion. To me the main attraction of the Laboratory, in contrast to universities, is its ability to pull together the resources to do a large multidisciplinary program and move on it quickly.

JOHN WHEATLEY: I received my doctorate from the University of Pittsburgh in 1952 and came here just recently, after stints at the University of Illinois and the University of California, San Diego, because I saw the opportunity to do both the basic physics research that is my main line of work and also what I call fundamental technology. That combination is highly regarded here, while in my previous university careers I always felt I had to sneak my interest in technology in the back door. After all, instruction through basic research, not development of technology, is the principal function of a university. Also, I perceive a very substantial increase in my effective mass here because the Lab has many more people interested in my field, which is thermal physics and condensed-matter physics.